

UNIVERSITY OF CAMBRIDGE DEPARTMENT OF PHYSICS

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March 13, 1953

Dear Joshua,

I am enclosing a copy of a MS by myself and Bill Hayes. The ideas presented in the paper are essentially those which arose immediately after the Fallenza meeting of last September. At that time I had hoped that we might obtain sufficient evidence to either prove or disprove the simple theory, but after six months, it became apparent that a complete proof (or disproof) would take a long time to achieve. We then thought it best to write up the present state in such a way as to suggest that some order does prevail. We do not think that our approach is at all complete since it seems probable that the results are biased by the occurrence of inversions, translocations, etc. I suspect, however, that such technical complications are not the cause of all of the difficulties and some such hypothesis as we have proposed will be found necessary to characterize the data.

We are unfortunately sticking our neck out and possibly shall have to bear the consequences. I have sent it to Delbrück for submission to the Proceedings of the National Academy and with luck it shall come out in April or May.

We have not mentioned the heterozygotes - This is on purpose since 1) we undoubtedly do not have access to all of your data since it is most likely that you have accumulated much new data since the publication of your 1951 C.S.H. paper; - thus any discussion of it by ourselves ~~would~~ could not hope to be complete and possibly completely wrong. 2) any discussion would need to be lengthy and would increase the length of the M.S. beyond PNAS size.

I have little doubt that our hypothesis will be in conflict with some of the relevant data. However, I believe it is necessary to have some hypothesis to work with since even if wrong, it does tend to

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new experiments are with luck to a more satisfactory hypothesis. It is in this mood that I send the MS to you, since from my viewpoint at least, our hypothesis achieves some degree of simplification. We have made no mention of possible experimental difficulties such as mixed photophores, selection in favor of some genotypes, etc. since it is our hope that these factors will to a certain extent cancel themselves out.

With luck, I shall be able to come to C.S.H. for the meeting, and at that time, I shall like to talk at great length with you about H-12. In any case, I shall return to the States in the fall to take up a fellowship at Caltech.

At present, I'm working entirely on the structure of DNA and have tentative hopes that we have cracked the problem of its structure. Pauling's attempt did not succeed. It is my hope to finish this the first stage of this work by the beginning of June. I would strongly guess that the solution of Gene Duplication will come from purely structural considerations and that the work on phage and the lower organisms though interesting in its own right will tell us nothing about the primary goal.

With best regards to both Estes and you

Jim Watson

P.S. I have just seen your 1952 Genetics paper on F<sup>r</sup> and shall change the appropriate references